Economic Assessment
Final Peer Review Report and Lead Agency Response

Report Content and Charge:
This report describes the peer review process and presents the lead agencies' response to the peer review. Also included are the names of the peer reviewers and their organizational affiliations, a compilation of all the peer review comments on the economic assessment reports, and the Principal Investigator's response to the peer review.

The peer review group (PRG) reviewed two economic assessment reports and one manuscript for publication, which are listed below. These reports were developed as supporting documents for the Programmatic Environmental Impact Statement (EIS) for Evaluating Oyster Restoration Alternatives for the Chesapeake Bay, Including the Use of Native and Non-Native Oysters.

1) Lipton, D., J. Kirkley, T. Murray. A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*. Prepared under contract for the Maryland Department of Natural Resources, January 2006.
2) Lipton, D. Final Draft Economic Analysis for Oyster Restoration Alternatives. Prepared under contract for the Maryland Department of Natural Resources, May 16, 2008.
3) Lipton, D. (2008) Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay. Journal of Shellfish Research: Vol. 27, No. 3 pp. 619-623.

The process followed for this peer review is consistent with the peer review plan that was developed by the Lead Agencies for the EIS project. This peer review plan was specifically designed to comply with the December 16, 2005 Office of Management and Budget's Peer Review Guidelines and was accepted by the US Army Corps of Engineers for this purpose.

Study Objective:
The objective of the Economic Assessment was to evaluate and assess the EIS alternatives based on economic effects. The assessment also had the goal of developing an economic impact statement to be coupled with the EIS.

The Lead Agencies Review and Response Process:
The Principal Investigator for the Economic Assessment, Douglas Lipton PhD, Department of Economics, University of Maryland College Park, worked with the PRG through three reviews to successfully complete the peer review process. The PRG provided comments on multiple draft reports and draft sections for the EIS and provided recommendations to guide the research and analyses. In addition, the Principal Investigator is anticipating the publication of findings from the economic assessment in a peer reviewed journal, "Lipton, D.W. Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay. Journal of Shellfish Research. Forthcoming 2008."

In November of 2007, the PRG completed their first review of reports for the Economic Assessment, including the January 2006 version of "A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*," and the May 2007 version of the paper "Background Paper: Economic Assessment of Aquaculture Alternatives to Restore Chesapeake Bay's Oyster Population."

In response to the peer review comments, the Principle Investigator revised the report, "Draft Economic Analysis for Oyster Restoration Alternatives" and developed sections of the EIS and the manuscript, "Economic Benefits of a Restored Fishery in the Chesapeake Bay," which were also peer reviewed.

In April of 2008, the PRG completed their second review of the economic assessment reports and draft sections of the EIS. The Principal Investigator addressed concerns raised in the second set of review comments by preparing a formal response to the peer review and by making edits to the reports "Draft Economic Analysis for Oyster Restoration Alternatives." He also edited the draft sections of the EIS and provided a new section of the Draft EIS, 5.6.2, for peer review.

The final peer review was completed in June 2008. The PRG stated that they were satisfied with the revisions made and felt that the documents were acceptable. The Lead Agencies are satisfied that the key concerns raised by the PRG during review of the Economic Assessment have been addressed.

The remainder of this report presents the PRG consensus review comments and the Principle Investigator's response.

Deposition of Peer Review:

Dr. James Anderson (Lead Reviewer)
Department of Environmental and Natural Resource Economics
University of Rhode Island

Mr. Keith Criddle
Fisheries Academic Program
University of Alaska Fairbanks

Dr. Walter Keithly
Department of Environmental Studies
Louisiana State University

Dr. James Opaluch
Department of Environmental & Naturals Resource Economics
University of Rhode Island

November 16, 2007 – First Peer Review Report

Peer Review Group Evaluation of:

Lipton D., J. Kirkley, T. Murray (January 2006). A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*.

And

Lipton D. (2007). Background Paper: Economic Assessment of Aquaculture Alternatives to Restore Chesapeake Bay's Oyster Population.

James L. Anderson, Peer Review Group Chair, University of Rhode Island

November 16, 2007

The Peer Review Group has completed its evaluation of the reports:

- Lipton D., J. Kirkley, T. Murray (January 2006). A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*.
- Lipton D. (2007). Background Paper: Economic Assessment of Aquaculture Alternatives to Restore Chesapeake Bay's Oyster Population.

The Peer Review Group considered the validity of the research design, the quality of data collection procedures, the robustness of methods employed, the appropriateness of the methods for the hypotheses being tested, the extent to which the conclusions follow from the analysis, and the strengths and limitations of the research projects.

The Peer Review Group came to relatively consistent conclusions regarding the reports. However, there are some differences in suggestions for improvement. In general, Lipton (2007) was well received, subject to the concerns related to the analysis found in Lipton, Kirkley and Murray (2006). In particular, the price analysis using an inverse demand equation found in Lipton, Kirkley and Murray (2006) is an essential component of the analysis found in Lipton (2007) and, therefore, has direct implications for the conclusions in Lipton (2007). As is clearly demonstrated in the following comments regarding Lipton, Kirkley and Murray, the Peer Review Group has reservations regarding the inverse demand equation estimation.

There are differences among the Peer Review Group regarding how best to address important concerns related to the estimation issues associated with the inverse demand model and use of the industry expert survey. One suggestion is to develop a more complete and/or respecificied price model. Attached is a recent report by Dedah *et al*. (2007), which may be helpful.

Alternatively, it was suggested that reliance on the inverse demand model be minimized and more weight be placed on the industry expert analysis. However, there is concern about whether industry can provide meaningful estimates when issues are outside recent historical norms. This concern also applies to the inverse price model. In any event, we recognize that the targeted production of 4.9 million bushels is outside production in recent history. Furthermore, it must be

recognized that the seafood industry and market are dynamic and adaptive. It is, therefore, very difficult to expect any model to forecast the price without considerable uncertainty.

A final issue, which was not addressed in the reports but may be highly relevant, is that of labor supply. The Chesapeake (and the Gulf) region has had problems attracting sufficient labor to the shucking/picking plants and has increasingly turned to foreign-sponsored labor. This is costly and likely places restrictions on the ability to process the potential increased oyster supply.

Lipton D., J. Kirkley, T. Murray (January 2006). A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*.

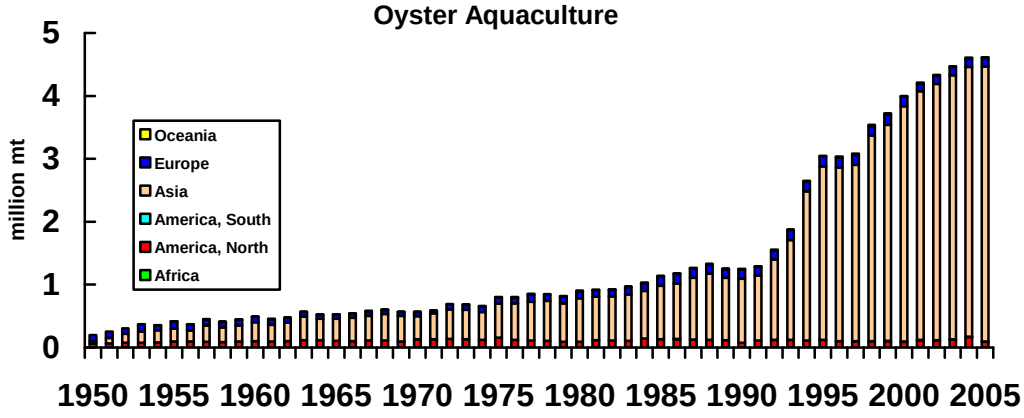
REVIEW 1:

The authors make several important observations:

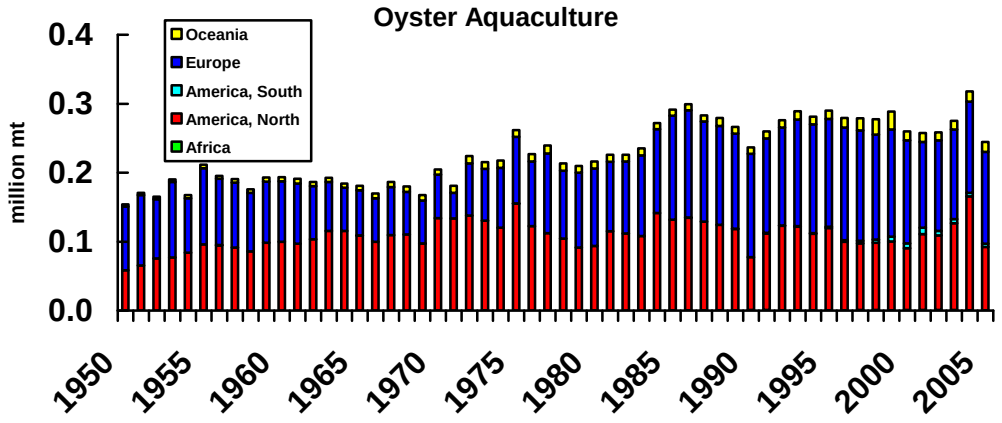
1. (Page 2) the population of oyster needed to sustain annual harvests of 4.9 million bushels may be greater than the populations present during 1920-1969.
2. Changes in net revenues to harvesters/farmers and processors will be less than changes in gross revenues.
3. Changes in benefits to the Chesapeake Bay area oyster fishery may be offset by changes in benefits to other US oyster fisheries. That is, even if the net benefits of an alternative are advantageous to the Chesapeake Bay area oyster fishery, they may not provide an increase in net benefits to the nation.
4. The shortened production cycle for triploid C. ariakensis increases value to producers because it reduces risk of loss to disease or untoward environmental conditions.
5. Changes in indirect benefits may be as large as or larger than changes in direct benefits.

Areas that should be revised include:

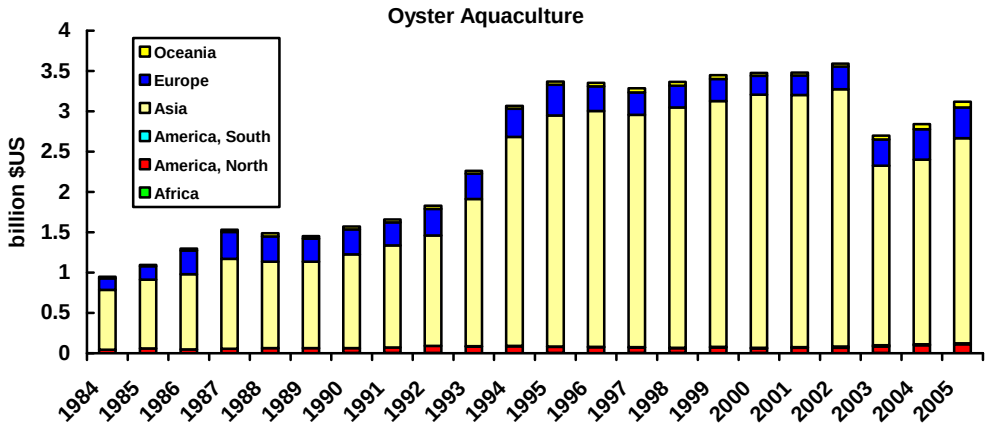
1. (Page 6) The text needs to address the possible role of international markets for oysters. Global aquaculture production of oysters has increased rapidly in recent decades.



Most of the growth has occurred in Asia with European and North American production holding nearly constant.

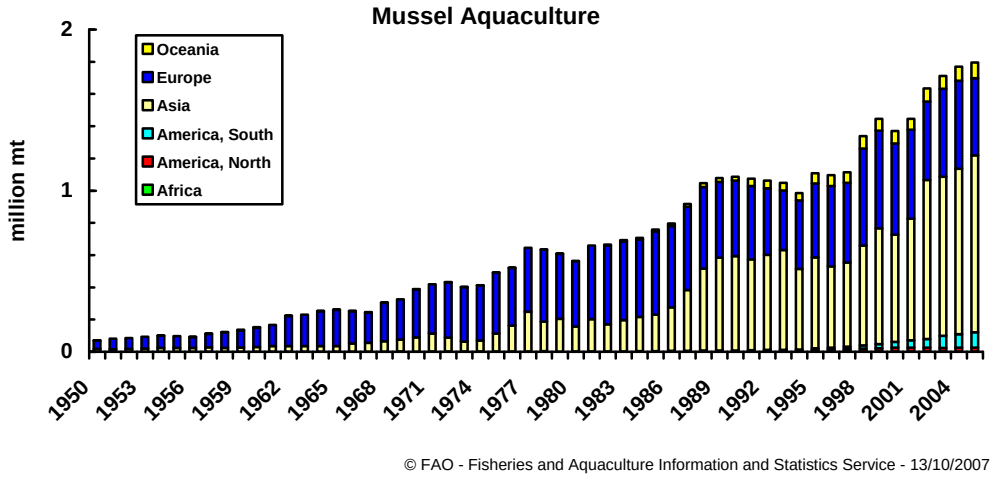


At the same time, the gross value of oyster production has held nearly constant.



An alternative hypothesis to account for the relative unresponsiveness of price to changes in US production is that US production has historically exceeded US demand, with surplus oysters sold into international markets. The unresponsiveness of price could simply reflect a reduction in the fraction of US production that is exported or in an increase in imports. This alternative hypothesis should be discussed. It could simply be that price is unresponsive because US production is an inconsequential contributor to international supply.

2. Discussion of the econometric model (page 8) should explain the limitations of a linear model specification (e.g., it allows for negative prices at high levels of production). The discussion should also address the decision to model this market in total levels rather than in per capita levels. In addition, the discussion needs to provide a rationale for and a discussion of the implications of failing to model imports, exports, inventories, and substitute productions. In particular, it should be noted that the recent rapid expansion of mussel aquaculture may have resulted in direct competition in US domestic markets for oyster or in international markets that have absorbed US exports.



3. (Page 11) The report should discuss similarities and differences between respondents and nonrespondents to the producer survey. Are they similar in production volume or are the nonrespondents larger or smaller than the respondents? Etc. Nonresponse bias can be difficult to rule out, but if observable characteristics of the nonrespondents are similar to those of respondents, concern about possible Nonresponse bias can be dismissed.
4. The discussion of Table 3 (page 13-14) discussion should also explain why there appears to be a premium for shucked C. ariakensis. (See e.g., page 31).
5. (Page 15) A possible explanation of the divergence between industry conjectures about the price flexibilities and the price flexibilities estimated in the regression model could be that industry leaders anticipate a nonlinear response or that industry leaders have different perceptions of the role of substitute products and export markets.
6. (Table 5) It would be helpful to express the gross value to processors/packers as the gross value net of payment to harvesters/producers.
7. (Page 26) The treatment of capital costs as annual expenditures is not unreasonable, but as the authors note, it is likely to lead to an underestimate of typical net revenues. Moreover, here and in subsequent pages, the discussion of comparisons between C. ariakensis and C. virginica production should be adjusted to reflect differences in growth rates. There are several ways that this could be addressed. For example, the costs of production of C. virginica could be inflated by the ratio of mean months to market size for C. ariakensis to mean month to market size for C. virginica. Alternatively, the time stream of net revenues for each species could be projected over a 10-20 year time line and the net present value of those net revenues could be contrasted.
8. The discussion beginning on page 30 needs to clearly emphasize that it refers to production of triploid C. ariakensis and triploid C. virginica and that production performance of diploids may differ from the production performance of triploids.
9. The concern expressed on pages 24/25, that permit requirements may have raised production costs is inconsistent with the remark in footnote 14 which suggests that growers may voluntarily reduce stocking densities in face of the more rapid growth of C. ariakensis.
10. (Page 31) The discussion of meat yields needs to clarify whether the reported yields for C. virginica are for triploids or for diploids.

11. (Page 32) The discussion of differences in shelf life misses the point that buyers are likely to be quickly educated about these differences and that buyer concern about shelf life will be reflected in market prices where it will be indistinguishable from differences associated with consumer perceptions of taste, palatability, etc.
12. (Page 34) For balance, the discussion of revenues in the capture fishery and revenues in the trial farms should also include a discussion of the net revenues of current aquaculture operations (discussed on page 33).
13. (Page 34 and subsequent pages) The references to total economic impact are not justified by the information reported. If estimates of regional economic impacts are to be retained in the report, there needs to be a discussion of how those impacts were derived and a clear discussion that regional economic impacts are not comparable with net revenues or net benefits and may simply reflect transfers of benefits between regions.
14. (Page 34) The projection of earnings to small-scale farmers in Virginia is not well motivated. If it is retained in the report, there needs to be an explanation for why it is assumed that there will be 24 producers and what assumptions go into projections about their operations.
15. (Page 39) The report appears to be missing the table referenced in the second paragraph.
16. (Page 41) The discussion of Hicks et al. (2004) does not provide sufficient basis for judging the reasonableness of the reported findings. If this document is part of the basis for decision-making, it should be subject to review. If it is not part of the decision record, but is important to the findings reported in this report, this report should be expanded to provide a more complete description of the methods and findings of Hicks et al. (2004). For example, what is the source of benefits? Did the study consider trade-offs among groups of anglers targeting different species? Did the study take into account models of population dynamics in the target and non-target fish species and the role that changing environmental conditions would have on that productivity? What was the sample frame of the survey? What are the confidence bounds on the reported estimates? Did the model account for nonlinearities in recreational demand or did it assume linear relationships? What did the model include as substitute recreational activities? What assumptions did the model make about changes in human populations and in the demographics of those populations?

Minor editorial notes:

1. Figure 3 should identify the units of measurement ($/lb meat) and the source of the data represented.
2. The description of model variables (page 8) should specify the units of measurement (e.g., $/lb meat, $/bushel) and whether the variables are nominal or real.
3. (Page 13 first line after Table 2). “their” should be “there”.
4. (Page 17 paragraph 2, 4th sentence) missing word: “above they” should probably be “above what they”.
5. (Page 22, 1st sentence on crab shedding tanks) missing word: “used to further nursery seed” should probably be “used to further growth of nursery seed”.
6. (Page 22 last sentence in last full paragraph) why would farms sell oysters to large shucking facilities to determine meat yields? Do these larger facilities provide some kind of certification that is relevant to direct marketing by smaller producers?

7. Table 6 seems unnecessary given Table 7. Moreover, Table 6 is missing information on average labor hours.
8. The information in footnote 10 (page 27) repeats information in the paragraph verbatim.
9. (Footnote 14, page 30) "bad" should be "bag".
10. (Page 38) Citation information is missing for reference to Grabowski et al (2004).
11. (Page 42 second paragraph) several problems with lack of agreement between verbs and subjects.
12. (Page 43) Citation information is missing for reference to Lynne et al (1974).
13. (Page 47) Murray (2005) is not cited in the report.

REVIEW 2:
Lipton, Kirkley and Murray (2006)

A. Document: A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*.

The stated purpose of the document is to "…summarize what is currently known about the economics of oyster restoration in order to inform the development of an economic impact statement." To this end, the authors first attempt to ascertain the direct benefits that might accrue to the oyster industry from the proposed action (restoring production to 4.9 million bushels). Then, the authors examine the performance of aquacultured *C. ariakensis* versus *C. virginica*; with discussion based primarily on results of field trials. Finally, the last section briefly summarizes available information related to other costs and benefits associated with oyster restoration.

Direct Benefits – the Oyster Fishery

General Comments:
In this section, the authors have the monumental task of addressing what the industry might look like under the restored fishery scenario (i.e., 4.9 million sacks or 34.3 million pounds of meats). To do so, an estimate of dockside price under the restored scenario conditions was first considered. This estimate was derived using two different methods: (a) development of an inverse demand model and (2) industry opinion. Both of these methods, of course, have certain limitations. These limitations, however, are somewhat "glanced over" in the report and I would suggest that increased attention is given to them.

Inverse Demand Model: The inverse demand model is based on annual data dating from 1950 through 2003. The authors acknowledge that "there is strong evidence that the demand for oysters in general has declined significantly over the last fifty years (p.6)" but do little to attempt to account for this declining demand (e.g., introduction of a trend variable) other than incorporating a structural shift variable representing the pre-1979 period.[1] Additionally,

[1] As a further indication of declining demand, the average price during the entire period of analysis when production averaged 17 million pounds was $3.53. Though production in 2003 was less than 5% of this average, the 2003

income does not appear to enter into the equation. I suspect that these omitted variables may be responsible for some of the apparently inconsistent results (e.g., Pacific Coast landings being a complement product).

Given the acknowledged changing demand for the harvested product, I would encourage the authors to use a shorter time frame for the analysis and incorporate additional demand shifters (e.g., relevant trend variables). They may also wish to examine imported product. Colleagues and I have recently estimated an inverse AIDS demand model for oysters for the 1985-2003 period based on quarterly data (sent as a separate attachment). We considered the three primary regions of production (Gulf, Chesapeake, and Pacific) and imports. Some of the primary results are given in the accompanying Table. As indicated, the price flexibility associated with the Chesapeake product is -0.76 (the same as for the Gulf product) which is roughly twice the magnitude of that estimated and used for analysis in the paper I have been asked to review (i.e., -0.37). Regardless, either estimate clearly suggests that commercial operations are likely to be economically infeasible at the level suggested by the restoration scenario.[2]

As one additional note, I would caution the authors not to read too much into changes in New England production and prices (p. 6). While it has been awhile, it is my recollection that the New England production increased significantly during the late 1980s due to a freshet associated with a tropical storm or hurricane that swept across the New England region in the mid-1980s. This freshet resulted in favorable conditions for a few years that yielded well above normal production which eventually subsided. It was not the high prices that resulted in increased production as implied by the authors.[3]

Table 4: Uncompensated Price Flexibilities [a]

	GUQ	CHQ	PAQ	IMQ	Scale
GUPR	-0.762* (0.045)	-0.037* (0.010)	-0.316* (0.036)	-0.130* (0.024)	-1.25* (0.067) -1.45* (0.409)
CHPR	-0.299 (0.222)	-0.759* (0.060)	-0.305* (0.139)	-0.082 (0.063)	
PAPR	-0.133 (0.068)	-0.003 (0.016)	-0.147* (0.064)	-0.102* (0.036)	-0.38* (0.109)
IMPR	-0.221* (0.079)	-0.023 (0.017)	-0.415* (0.072)	-0.221* (0.079)	-1.07* (0.128)

dockside price was only 15% above the long-run average. This would imply either an extremely low price flexibility (doubtful) or a significant change in tastes and preferences (much more likely).

[2] I might mention that we have recently estimated variable costs of production from private leases in Louisiana (using a translog approach) and variable costs approach $8.00 per sack, on average. Given the extensive nature of Louisiana's operations, I would anticipate substantially higher costs associated with the more intensive systems being considered in the Chesapeake. I can provide the committee a copy of this report if it might be useful.

[3] Additionally, I have always questioned exactly what the prices represented (prices at wholesale or dockside).

Note: Quantities for the Gulf, Chesapeake, Pacific, and imported oysters are represented by "GUQ"; "CHQ"; "PAQ"; and "IMQ," respectively. Prices for the Gulf, Chesapeake, Pacific, and imported oysters are represented by "GUPR"; "CHPR"; "PAPR"; and "IMPR," respectively.

a The standard errors are in parentheses.

*** indicates parameter estimate is significant at the 5% level.**

An Industry-Informed Scenario of a Restored Oyster Fishery:

(a) In an attempt to gain further insight into what a market might look like from a restored oyster fishery in the Chesapeake Bay, dealers in Maryland and Virginia were surveyed regarding their individual perceptions of price and other factors if production were to increase to 4.9 million sacks.[4] Results of the survey indicate that respondents believed that the large increase in production (i.e., from less than 300 thousand pounds in 2003 to more than 30 million pounds would result in only a relatively minor reduction in dockside price (i.e., from the current $24 per sack to about $18 per sack).

I must admit that I have serious concerns regarding the ability of industry to provide meaningful information pertaining to potential impacts associated with changes well outside historical norms. As such, I am somewhat skeptical of the findings presented in relation to expected prices paid to harvesters and tend to discount them. While I also have some concerns regarding the inverse demand model (as discussed above), I believe the findings associated with this model are somewhat more reliable than findings associated with the Industry-informed scenario.

Overall, I suspect that the market will only absorb some fraction of the 4.9 million sacks and that the remaining amount will not be harvested (thereby potentially providing additional non-market benefits). Without additional information, however, one cannot begin to estimate what quantity would be harvested if the restored stock was large enough to permit a sustainable 4.9 million sack annual harvest.

(b) Results pertaining to the current percent of product that goes to halfshell trade (p. 12) is confusing and, potentially, misleading. If you look at the NMFS end-of-the-year processing data, one can ascertain that processing activities in the Chesapeake is several times larger than production in the region. This would imply that a large amount of product is imported into the region for processing. While I have not attempted to verify it, I suspect that most Chesapeake processing activities entails Gulf product while the locally harvested product is destined for the halfshell trade. However, increases in Chesapeake harvest would certainly lead to a significant portion of the harvest being shucked.[5] The industry opinion is that restoring the fishery to historical levels would result in 22% to 30% of the harvested product being directed towards the halfshell market (with the difference depending upon the species though, admittedly, the difference between the two is not likely to be statistically significant)[6]. Given my belief that the harvest of 4.9 million sacks is economically infeasible, I can't make any definitive comments regarding the accuracy of industry expectations. However, I do think that industry opinion may

[4] Note: in the survey that is attached at the end of the report, the first three questions elicited information regarding restoration to 4.9 million sacks. The remaining questions elicited information regarding restoration to 2-3 million sacks. No explanation for the two alternative restoration scenarios is provided.

[5] If I am not mistaken, well in excess of 70% of the Gulf product has historically been processed.

[6] While the *C ariakensis* certainly relates to triploid production, it is unclear whether *C. virginica* also refers to triploid production. This may be relevant in determining what percent is destined for shucking.

be relatively accurate for a relatively large increase in production (say, equivalent to Gulf production). [7] Having said this, however, the very short shelf life for the *C. ariakensis* halfshell product would indicate that a very large proportion of it would need to be directed to processing.

The wholesale prices reported by the industry (Table 2) are inflated, in my opinion, due to the "optimistic" price paid to the harvesters. While these prices are inflated, the industry expectations of profits (Table 3) are likely to be somewhat reflective of conditions that are likely to hold with a "significant" increase in Chesapeake harvest (though well below the 4.9 million sacks). As would be expected, the processors expect to receive a higher per unit profit for the *C. ariakensis* processed product than for the *C. virginica* processed product. This is reasonable given the larger size of the former.[8] Less reasonable, however, is the discount given to cultured processed product.

The discussion of revenues from the two products *C. ariakensis* and *C. virginica* are, again in my opinion, very unrealistic because they are based on an output level that, while possibly sustainable from a biological perspective, is not attainable from an economic perspective. Finally, question #8 on the survey indicates that dealers were asked whether they have had business-related experience with *C. ariakensis*. Yet, I do not see any discussion associated with this question. Given the extremely limited shelf life for the *C. ariakensis* used in the halfshell trade (as discussed in a later section of the report), it would be interesting to note if there are differences in beliefs among the dealers familiar with the product versus those not familiar with the product.

Combining the Industry Scenario with the Inverse Demand Analysis

Frankly, the analysis presented in this section appears illogical to me. The premise is that industry may have understated the reduction in price that would be forthcoming with a restored fishery. As a result, using the industry estimate of price (at 4.9 billion bushels), the investigators then determine what level of output would generate that price; based on the inverse demand model developed in the first section of the report. Solving for quantity yields an estimate of about 18 million pounds of meats, or 2.57 million bushels.[9] Strangely enough, while the basis for this estimation process appears illogical, the outcome does appear somewhat reasonable. It is roughly equal to the Gulf of Mexico production and likely could be marketed in a timely manner.

Welfare Implications of the Commercial Fishery

Based upon the 2.57 million sack scenario, this section appears to do an adequate job of attempting to examine welfare implications. It is relatively complete (given the paucity of information) and I have only two concerns/questions. First, industry expectations of profits based on product form was given in Table 4. I don't know why this was not used to provide, admittedly, a rough estimate of net benefits to Processors/Packers in Table 5. Second, given my concerns with the inverse demand specification, these concerns transcend to the estimate of consumer surplus.

Specific Comments:

[7] It might be insightful to compare the NMFS processing data in the 1970s to Chesapeake landings to examine the role of processing when harvests were more reflective of historical practices.

[8] The *C ariakensis* product will be triploid. It is unclear from the questionnaire whether the *C. virginica* will also be triploid. If so, one might question the profit premium on the former.

[9] This estimate approximates the restored fishery conditions in the second set of questions in the survey (i.e., 2-3 million bushels).

I have only two specific comments that might be of value for this section.

1. Harvesting costs appear to be the major limitation to the analysis. Estimation of these costs is complicated by the fact that cost per unit of output varies with the underlying population of oysters and the management regime (i.e., open access versus leasing). Nonetheless, I would recommend placing some upper and lower bounds on harvesting costs. The information in Table 6 may provide some information on upper-bound costs (approximately $27 per sack based on 220 oysters per sack). This number is probably exceedingly high since it equates to $3.85 per sack, which is roughly the current dockside price. However, you may be able to adjust the figure based, say, only on the more efficient firms. Similarly, a lower-bound estimate could be derived (from, say, the recently completed Louisiana study which suggests variable costs of approximately $8 per sack[10]).

   Based on lower-and-upper bound estimates of costs per sack, one could then use the inverse demand model to examine potential lower-bound and upper-bound break-even number of sacks. Welfare analysis could then be conducted on these figures.

2. The authors of the document appear to be under the perception that the EIS presupposes a harvest of 4.9 million sacks. I believe the critical point is that the restored population would allow 4.9 million sacks to be harvested. It is highly unlikely, in my opinion, that 4.9 million sacks would be harvested due to cost and other constraints. In Louisiana, for example, if product is not moving, harvesters will at times be placed on trip limits. I would expect the same for the Chesapeake. Adding to this is the fact that the shelf life for the *C. ariakensis* product is extremely limited. As such, markets are also likely to be a more limiting factor that will place constraints on quantity supplied.

Virginia Seafood Council Triploid *Ariakensis* Trials

This section of the report is relatively straightforward and merely reports on results of various field trials and marketing activities. I have only some minor questions/concerns that may be of relevancy.

1. Implied labor costs represent a relatively large share of total cost (approximately 37%). In fact, only the total number of hours of labor was recorded and an assumed wage of $10 per hour was used to calculate total labor costs. Given that total costs are, to some extent, driven by labor costs, I think some substantiation of the $10 per hour used in the analysis would be warranted.

2. It is my understanding that the nine different operations (eight if Yorktown is considered a single operation) each used different methodologies (e.g., bags on racks, long-lined bags on bottom). If possible, I would suggest deleting the crab shedding technique from the cost and returns estimate. This technique is land based and, hence, appears to be outside the objectives set forth in the EIS (it is unclear to me whether the crab shedding technique is used only for the production of seed oyster).

3. It is unclear to me what the costs presented in Table 7 (referred to as table 1 in the text) represent. The early discussion suggests that both triploid *C. virginica* and triploid *C. ariakensis* were raised in the field trials. From what I can ascertain, however, cost and

[10] This figure could be adjusted for differences in sacks per trip with lower harvests equating to higher harvesting costs per sack.

return estimates reflect only the later product. While I understand why revenues may only reflect *C. ariakensis* (shorter growing time which allowed them to be marketed within the study period), I don't see why there is no information on costs associated with *C. virginica*. It would be useful to the reader if some additional information is presented here.

4. As written on p.30, "[b]ased upon these pilot demonstration projects, it is evident that a profit can be made with triploid *C. ariakensis* aquaculture." This is true when only 3,000 sacks are produced. However, one would expect significant production to result in a price decline. This then calls into question profitability at levels of production more consistent with objectives (again, while I cannot foresee 4.9 million sacks being produced, the goal would certainly be to produce more than 3,000 sacks). I think one needs to caution the reader that increased production is likely to result in lower prices that would then call into question long-run profitability.
5. With respect to the high yield from shucking, I wonder if this is the result of using triploids or the result of the species being considered. Is there any information yet available on yield from triploid *C. virginica*? If the information is now available, I would suggest including it in this section.
6. The fact that *C. ariakensis* has a very short shelf life associated with the half shell product calls into question whether large quantities of it could be marketed for the half shell market if successfully produced. It is my understanding that under proper conditions, *C. virginica* will last for a minimum of 10-14 days. This finding suggests to me that any half shell market for the *C. ariakensis* product would, by necessity, need to be local which would significantly limit total demand.
7. Similarly, the shelf life of shucked *C. ariakensis* product would be useful to know. If relatively short vis-à-vis *C. virginica*, one would expect only limited demand. If this information is now available, I would encourage the authors to include it in the final report.
8. What is the definition of an oyster farm (p.36)? If it is a lessee, I would think that the state would maintain some records of the number of operations.

Indirect Use and Non-use Benefits

Without reviewing the Hicks *et al.* (2004) study in some detail, I have little to offer on this section of the report. The review that is provided appears factual and clearly introduces some of the limitations of the report.

Based upon this review, we can now consider the criteria noted above:

● Are the conclusions adequately supported by the evidence, analysis and argument?

In general, conclusions are adequately supported by the evidence. There are several instances, however, where I believe the investigators have drawn conclusions not clearly supported by the evidence. Having read through the report, I am not at all convinced that introduction of *C. ariakensis* would support any large-scale commercial operation (say, 1.0 million sacks), even if biologically successful. While it may support a "cottage" industry (a few dozen small-scale producers to even one-hundred producers), the information provided in the report does not provide sufficient evidence that any significant direct benefits would be forthcoming. Of course,

however, the indirect benefits that may be forthcoming may be large vis-à-vis any direct benefits. The paucity of information, however, limits examination of these potential indirect benefits.

● Are the uncertainties or incompleteness in the evidence explicitly recognized?

It is my opinion that uncertainties and incompleteness in the evidence has been overlooked in certain instances. Many instances have been noted. Having made this comment, however, the investigators were given a daunting task given the limited amount of information available to them for analysis.

●Are the data and analyses adequately described and well-suited to address the questions being investigated?

In general, the data and analyses are adequately described. Some areas of omission have been noted above.

● Are statistical methods applied appropriately?

Overall, I would conclude that statistical methods could be improved upon. This includes both the inverse demand model and analysis associated with the survey discussion. Concerns with the inverse demand model have been discussed and are not repeated here. Concerns with the survey discussion reflect two main areas. First, the authors should more adequately note the small sample size of the survey and examine the relevant findings in relation to this small sample size (possibly including standard errors). Second, the investigators make certain assertions that would likely not be upheld based upon statistical analysis of the data. For example, the reported price/profit differences associated with the different products (Tables 2 and 3) are likely not statistically different if the appropriate hypothesis testing were conducted.

● Is the report fair and impartial in tone?

Yes.

REVIEW 3:

Comments on

"A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*."

Douglas Lipton, James Kirkley and Thomas Murray

This report contains much potentially useful information on the economics of oyster fishing in the Chesapeake Bay, including demand modeling, an industry survey, a financial analysis of oyster processing operations and an estimate of consumer surplus from increased oyster production. The paper generally focuses on appropriate measures of economic surplus, but is unable to provide estimates of some categories of surplus primary because of inadequate cost data for watermen and for processors.

The demand analysis is used to forecast oyster prices in Chesapeake Bay for a scenario where oyster production is increased from current levels to a target level of 4.9 million bushels, or 34.2 million pounds of meat. The demand model estimates that market price would fall to $1.51 per pound or $10.58 per bushel at this production target. The paper concludes that this is not likely

to be profitable as these prices are far below historic levels, however, the paper does not have adequate cost data to do a full analysis of economic feasibility.

The demand estimation employs a simplified, single equation model, estimating the inverse demand function (price as a function of quantity). As the paper acknowledges, a more complete analysis would estimate a full system of demand and supply. In many cases, fisheries demand analyses are carried out with single equation systems, assuming production is predetermined, which may be a good approximation for a short run analysis. However, whether this is appropriate with annual data is an open empirical question, and one that is not addressed in the report.

However, there is a significant problem with the empirical estimates that leads me to question the validity of the demand analysis. In the regression coefficients shows in Table 1 on page 9, the coefficients on quantity of oysters supplied from the South Atlantic and the Pacific Coast are highly statistically significant, but are of the wrong sign. These results imply that price of Chesapeake oysters would *increase* with increasing supply from these two regions. The result is not sensible, and should be taken as a sign that there is a serious problem with the statistical results, so that forecasts made with this demand model are not reliable.

Some alternative statistical model formulations might include:

- Re-estimate the model as a simultaneous system of supply and demand, perhaps using shorter time intervals (e.g., monthly data).
- Estimate supply function of *fishing effort*, rather than catch, then calculate harvest using h = qEX. This might be statistically estimated, or it might be treated as an identity.
- Re-estimate the model as a recursive system, with catch (or effort) being modeled as a predetermined function of lagged prices.
- Estimate a model of national price as a function of national catch, rather than trying to model Chesapeake bay price as a function of regional quantities.
- One might use a partial adjustment model for supply (effort or quantity), based on the notion that watermen have limited outside opportunities and/or fixed capital stocks that are fixed in the short run.

I'm sure there are other options that might be tried. In any case, supply estimation should be conditioned on the estimated size of the oyster population in the relevant time period, since there is no reason to believe that the data clearly do *not* represent bioeconomic equilibria. In the absence of more reliable statistical results, I would recommend that the analysis be based on the results of the expert survey, and that the demand modeling not be used for price forecasts.

On pages 6-7, the paper reports strong evidence that demand has declined significantly over the past 50 years. Yet the model doesn't include a time trend on demand. Rather the modeling employs a limited number of dummy variables to capture specific incidents which may have affected demand. I recommend that the model include a time trend in the structural demand model.

Lipton D. (2007). Background Paper: Economic Assessment of Aquaculture Alternatives to Restore Chesapeake Bay's Oyster Population.

REVIEW 1:

It would be difficult to follow this document without first reading Lipton, Kirkley, and Murray (2006). Even with that background, this report is sometimes difficult to follow because it keeps switching between information expressed in bushels and information expressed in individual oysters. Nevertheless, subject to some concerns expressed below, the general modeling approach seems appropriate for the contrasting the performance of alternative aquaculture scenarios.

Areas that should be revised include:

1. The report should use consistent units of measurement for describing input and output prices and quantities across all the scenarios.
2. The report (page 6) should explain why growth is assumed to be faster under the second scenario (diploid *C. virginica* hatchery seed on-bottom). I imagine that this is due to disease resistance that allows for higher growth rates than are observed in the wild stock, but this reasoning is not provided in the report.
3. The report does not provide equally detailed discussion of the 8 scenarios. Although the results for all 8 scenarios are reported in Table 3, it would be helpful to have a more complete discussion of each scenario, or at least an explanation for why some scenarios were not discussed in detail.
4. The report should explain why spat costs for triploid *C. ariakensis* are appropriate for inclusion in a simulation of wild *C. virginica* raft culture (page 7). Is there no independent estimate of the cost of wild *C. virginica* spat? Are the costs of producing diploids and triploids identical? Are there any surcharges for patented technology in the production of triploids?
5. (Page 8) The base simulation is described as assuming a mean price of $0.25 with a standard deviation of 20%. Since the simulations embed the econometric model described in Lipton, Kirkley, and Murray (2006), it would be more appropriate to use the 80% prediction interval on price evaluated around the assumed price of $0.25.
6. Because the simulations are predicated on the econometric model described in Lipton, Kirkley, and Murray (2006), the results are subject to the concerns about model specification discussed in my review of Lipton, Kirkley, and Murray (2006).
7. (Page 18) It is not clear why smaller oysters (300/bu instead of 225/bu) would result in an increase in the number of profitable firms. This result should be carefully investigated to ensure that it reflects lower average prices for smaller-sized oysters.
8. (Page 18 conclusions) The conclusion that the EIS goals will not be met by an increase in private aquaculture needs to be better motivated. At a minimum, the report should remind readers that the EIS goal is conditions that would result in annual harvests of 4.9Mbu while the simulation suggests a mean harvest of 3.23Mbu. It would be helpful if the report included a table that included estimated mean harvests and confidence intervals under each of the 8 scenarios. This would provide readers with a sense of the probability that the EIS objective would be reached under each scenario. To further represent the

uncertainty of the simulations, it would be helpful to include additional tables that express the likely distribution of harvests under varying levels of oyster survival and prices.

Minor editorial notes:

1. (Page 5, paragraph 2, sentence 3) Should this read "The scenario is for a grower to *plant* 10,000 bushels of seed oysters …" ?
2. (Page 8 line 6) text should be qualified to say "cages and rafts *are assumed to* have an economic life …"

REVIEW 2:

Economic Assessment of Aquaculture Alternatives to Restore Chesapeake Bay's Oyster Population

General Comments:

1. This is a very well written document and the methodology employed, while simple, is adequate for the analysis. Of course, the results will reflect the values employed in AQUASIM and the investigator uses those values available to him. To the extent that values used do not represent those an aquaculture operation would face, however, the results will likely be misleading. Given the limited amount of information on each of the eight alternative systems, however, it is difficult to ascertain whether the values used in the analyses represent those faced by a representative firm. The only question I have with the initial runs relates to output prices. Initially, $30.00 per bushel for harvested oysters was assumed (i.e., under the Diploid *C. virginica* Wild Seed On-Bottom Scenario). With the Native Oyster Raft Culture Simulation, however, an output price of $55 per sack (i.e., $0.25 per oyster @ 225 oysters per sack) was assumed. Is there some basis for suggesting that native oysters produced using a raft culture technique would command such a premium over those produced using the on-bottom technique? I would imagine that one could better control size and shape with the raft technique but I think the issue of different output prices needs to be further discussed. (note: there is additional discussion of this issue on pp.14-15, but no reason for the different prices is provided).
2. Given uncertainties with a number of the cost elements (e.g., labor and seed and interest rates), it would be useful to perform some sensitivity analyses associated with some of these key elements.
3. As indicated in the Diploid *C. virginica* Hatchery Seed On-Bottom Scenario, "survival would need to improve to about 16% for the enterprise to break-even." This survival rate sounds very high to me (even with the use of disease resistant strain) given the on-bottom conditions. Is there any information regarding what the expected survival rate is under this scenario?
4. The issue of a $10 per hour labor costs was raised in the review of the previous study. I note on p. 7 that $10 per hour labor cost was use in a study published 15 years ago. It would appear to me that unless the authors can justify use of this figure, it should be adjusted upwards to reflect current labor costs in the region. Given that labor costs

represent a relatively large share of total costs, a change in the labor costs is likely to significantly influence results.

5. With respect to the Simulation of triploid *C. ariakensis* production based on data from VSC trials scenario, I have a difficult time following how "[t]he variance in costs and returns data from the 9 operations was used to generate the distribution parameters for the Aquasim model (p.10)." Doing so would be reasonable if the operation scale was not aggregated across the nine enterprises. Given the aggregation, however, I would think that the variance would increase by some (possibly large) amount.
6. Just as an aside, I expect record Louisiana production during the next couple of years. History has shown that hurricanes result in a short-term disruption in production (due to initial mortality associated with excessive fresh water) but large increases after about two years.
7. As noted, I cannot agree with the estimate that price will decline by only 22% with a very significant increase in production (anywhere from 2.57 million bushels to 3.23 million bushels. Hence, while the analysis conducted with respect to overall production levels is theoretically sound, I believe it is seriously flawed. My concerns with this estimate of price decline were discussed in the previous paper.
8. Finally, I suspect that applying the $0.24 price currently received by the Virginia Seafood Council trial to any large output (e.g., 22% of 2.57 million sacks) is unrealistic due to reasons stated above. Among the main reasons, the very short shelf life of the product will severely limit development of anything but a local market.

Based upon this review, we can now consider the criteria noted above:

● Are the conclusions adequately supported by the evidence, analysis and argument?

See discussion. I am primarily concerned with the results associated with expansion from a trial run to a large industry output.

● Are the uncertainties or incompleteness in the evidence explicitly recognized?

To some extent the uncertainties are recognized. As noted, however, I think additional sensitivity runs with respect to items listed above would be useful and would provide additional insight.

●Are the data and analyses adequately described and well-suited to address the questions being investigated?

The investigator, to my knowledge, used the only data that were available to address the questions being investigated. While the sources of the data were adequately described, it might be useful to include an appendix with a more complete description of the data. Finally, it is difficult to address whether the data are well-suited to address the questions being investigated. Given the fact that relevant data are extremely limited, the investigator made very good use of available information. Having made this comment, one would always like more than results from a single experiment (some more than 15 years old) from which to "borrow" data for subsequent analysis.

● Is the report fair and impartial in tone?

Yes.

April 27, 2008 - Second Peer Review Report

(Please note that the Principal Investigator provided edited documents and additional documents to the peer review group in response to the first peer review)

Lipton D. (2008). Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay
Lipton, D. (3/16/2008). Final Draft Economic Analysis for Oyster Restoration Alternatives
and
Lipton D. (2008). Background Supporting Document B - For Economic Analysis of Alternatives: Projecting Oyster Harvests

James L. Anderson, Peer Review Group Chair
University of Rhode Island

April 27, 2008

The Peer Review Group has considered the manuscripts submitted by Lipton in response to the review panel's comments of 11/16/2007.

- Lipton D. (2008). Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay
- Lipton, D. (3/16/2008). Final Draft Economic Analysis for Oyster Restoration Alternatives
- Lipton D. (2008). Background Supporting Document B - For Economic Analysis of Alternatives: Projecting Oyster Harvests

The Peer Review Group had some difficulty in clearly determining which aspects of the prior work were revised because traditional point-by-point response was not provided. Furthermore, two documents were provided initially, while the response contained three documents.

As before, the new documents were reviewed considering the validity of the research design, the quality of data collection procedures, the robustness of methods employed, the appropriateness of the methods for the hypotheses being tested, the extent to which the conclusions follow from the analysis, and the strengths and limitations of the research projects.

In general, revised documents are well done; however, there is still concern regarding the price analysis using an inverse demand equation and the industry expert-based analysis. It is an essential component of the analysis and, therefore, has direct implications for the conclusions.

There is still concern whether industry can provide meaningful estimates when issues are outside recent historical norms. This concern also applies to the inverse price model. In any event, we recognize that the targeted production of 4.9 million bushels is outside production in recent history.

REVIEW 1:
Review: Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay

I noted several concerns that I had in a similar draft of this manuscript, and they have not been addressed as far as I can determine. The inverse demand equation specifies the annual Chesapeake Bay price as a function of harvests in various regions (the Chesapeake, the Gulf, the Mid Atlantic, the Northeast, the South Atlantic, and the Pacific), a discrete variable taking a value of 0 for 1950-78, and an interaction term corresponding to a hypothesized impact of Vv on Gulf product. As noted in my previous comments, I do not see the logic of omitting income (either real or nominal depending on the specification of price) from the equation. As such, the model violates an important theoretical consideration. Similarly, imports are not included in the specification. If imported product competes with domestic product, omission of this variable will result in bias among the parameters associated with the included variables.

Should income and imports be included? Certainly, the fact that Pacific product is found to be a complement should raise a "red flag." This is particularly the case given that this region is the second largest producer, behind the Gulf. While I would find plausible results that suggest that Pacific product does not statistically influence Chesapeake price, a conclusion that Pacific product is a strong complement to the Chesapeake product is simply implausible, in my opinion. This "unusual" finding, in my opinion, is the result of model misspecification.

I would suggest the following avenues to determine the validity of the results. First, and most relevant, income needs to be included in the model. Second, imports need to be considered (even if it entails using a shorter time period). Third, I would suggest looking at the sensitivity of the model results to the time period being considered. I would have argued against using 50+ years of data in any demand analysis and this is particularly the case for oysters. Given all of the changes in the consumer perception of the product over time, I would have suggested using the shortest time period possible for the analysis (even if this were to entail deleting or aggregating across some of the less relevant regions where production has historically been minor). Running the analysis from 1980 forward would still provide 25 years of data and this should be sufficient for the analysis in the absence of serious multicollinearity. In short, are the results robust to the starting/ending point? If not, the provided results may have little meaning.

With respect to the industry survey, I stand by my previous review that industry is, by and large, unable to make predictions of what prices would be for an amount of product (4.9 million bushels) that has not been observed in the Chesapeake since the 1950s.

Finally, as I mentioned in my comments on another draft on this subject, the authors appear to use a *non sequitur* line of reasoning when stating, "[a] reasonable approach would be to take the industry price predictions and then determine what size fishery would result from those prices (from the inverse demand model) (p. 12)." The flaw in this line of reasoning is as follows: (a) there appears to be a problem with the inverse demand model in predicting price at 4.9 million bushels; (b) as a result of this problem, we will use industry input to determine the expected price; (c) the industry prediction shows that increased production will result in only a relatively small reduction in price vis-à-vis the inverse demand equation; (d) while the price reduction provided by industry is associated with a new production level of 4.9 million bushels, we don't have much assurance in the outcome; (e) therefore, we will use the price that they foresee for the industry at 4.9 million sacks to determine what the "real" quantity would be using the inverse demand model that we tended to "glance over" because we had serious reservations about how price reacts to the quantity being supplied.

In short, I have serious reservations regarding the manuscript and any policy that might be based on what is reported in the manuscript. While the own-price flexibility estimate of -0.37 may be "close" to the true number, there is little in the manuscript that gives me any assurance. Furthermore, since own-price flexibility is one of the key parameters that will determine the feasibility of any of the options, I encourage the authors to go back and spend some time rethinking the basic inverse demand model and examining its sensitivity to alternative specifications/starting points. This is, after all, much more than an academic exercise. Conclusions are likely to lead to considerable investment (or lack of investment) by the state and federal governments and, as such, it is best to spend some additional time upfront.

REVIEW 2:

I read through the Lipton article, and I don't see any response to my original concerns. Most importantly, the estimated coefficients on harvest from the Pacific and South Atlantic regions in the inverse demand function have the wrong sign. Thus, an increase in catch from either of these regions would INCREASE the price received for oysters caught in the Chesapeake. Not only are these coefficients the wrong sign and highly statistically significant, but they are also the largest harvest coefficients in absolute value. So according to the estimated equation, catch from these two regions is quantitatively more important than harvest from any other region, including the Chesapeake. For example, if quantity supplied from all regions simultaneously increased by 10 million bushels, the model would predict that price in the Chesapeake would increase. There is no reason to believe that oysters from other regions are complementary to oysters in the Chesapeake nor that harvest from the Pacific and South Atlantic are more important determinants of price than harvest within the Chesapeake.

One might argue that this is not a problem for calculaing the price flexibility of supply in the Chesapeake because the coefficients on Pacific and South Atlantic harvest are not part of calculating the price flexibility, and the own-price flexibility in the Chesapeake is of the correct sign. However, there is a broader concern that these anomalous results raise questions about the credibility of the entire model.

I stick with my original assessment. I think it is potentially a good paper that has important analyses that could be of direct policy relevance. However, I would have concerns about using the coefficient estimates from the inverse demand function to guide policy given the anomalous results.

REVIEW 3:

Review notes on Lipton (2008)—Final Draft Economic Analysis for Oyster Restoration Alternatives, Lipton (2008)—Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay, and Lipton (2008)— Background Supporting Document B - For Economic Analysis of Alternatives: Projecting Oyster Harvests..

Although a few sections of the "Final Draft Economic Analysis for Oyster Restoration Alternatives" are incomplete and there are some minor grammatical flaws, the analysis is fundamentally sound and is responsive to concerns flagged in the Peer Review Group Evaluation of: Lipton D., J. Kirkley, T. Murray (January 2006)—A Background Economic Analysis for the

Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis* and Lipton D. (2007)—Background Paper: Economic Assessment of Aquaculture Alternatives to Restore Chesapeake Bay's Oyster Population our previous review.

The econometric model and accompanying discussion in "Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay" need to be updated to reflect the revisions reported in "Background Supporting Document B - For Economic Analysis of Alternatives: Projecting Oyster Harvests;" e.g., the revised price flexibility estimate of -0.39. After these revisions have been incorporated, the analysis will have adequately addressed concerns flagged in the Peer Review Group Evaluation.

May 7, 2008 – Response to Peer Review

Response to Peer Review Comments of April 27, 2008

The peer review comments shared similar concerns, and thus, will be responded to in a combined response rather than repeating a similar response to each reviewer. The peer review comments raise legitimate and important concerns that need to be addressed. The comments are mainly concerned with two critical issues: 1) the specification of the inverse demand model; and 2) the use of the industry survey to determine equilibrium restored industry price, and then solve the inverse demand for the corresponding projected restored industry size.

Use of Industry Survey

The latter concern about use of the industry survey is most easily dealt with so that response will be presented first. In fact, in response to the first round of peer review comments, the industry survey data was only used in the EIS to validate the alternative approach of using the minimum observed oyster price over the time series as an indicator of minimum average production cost, and thus, a potential long-run equilibrium price for a restored oyster industry. The minimum price of $20.07 per bushel occurred in 1974. The industry survey median price response in adjusted dollars was $19.36 to the question of what the expected price of a restored oyster fishery would be. The fact that the industry survey data, as limited as these projections could be, was within 4% of the minimum price provided a little more confidence in using the $20.07 price as the equilibrium price. The justification for using either the survey data or the minimum price was not that there was "a flaw" in the inverse demand model. We used the inverse demand model to demonstrate that the policy of trying to restore an industry that would harvest 4.9 million bushels was flawed in that it would result in oyster prices well below historical minimum levels, and thus, below production costs. We then set out to determine what the long-run equilibrium price and quantity would be from a restored oyster resource that was capable of increased sustainable harvests. The quantity estimate would be obtained from the inverse demand model, modified as discussed below.

Specification of Inverse Demand

The concerns about the specification of the inverse demand require more detailed discussion. Several changes were made based on the earlier review comments including using per capita values to account for population growth, and using a time trend variable in place of a discrete (0,1) demand shifter. Data were also obtained on oyster imports and the model was run incorporating that data, but the parameter estimates were not significant. Once the model results, after these few adjustments, were confirmed to not be significantly different than the Dedah et al.

results; the process of revision ended (prematurely). As the reviewers point at in this round of comments, additional concerns should have been addressed, particularly the addition of an income variable and some other issues.

In response to these latest comments, I have revised the inverse demand specification in two significant ways that I believe address the major concerns. First, I added an annual real disposal income variable. The results (Table 1) show a significant and, as expected, positive relationship. Second, I re-examined my approach to including imports. Previously, I had included all oyster imports into a single product. Oyster imports are dominated by canned and smoked product, although fresh and frozen have increased significantly in the past few years (see Figure 1). Thinking that this aggregation may have masked some change in the market structure, I re-ran the model using just imports of "fresh and frozen" or "fresh/frozen/salted/brine", depending on the year and the classification code in use at the time. As with the previous experience, imports were not significant, although they did have the expected sign (Table 1).

However, rather than going back to the previous specification using the longer time series without imports, this model was adopted as the final specification because it addresses many of the concerns of reviewer #1, particularly in regard to the length of the time series and stability of demand. The model used is then:

$$(1)\ P_{ch} = \alpha + \beta_1 X_{ch} + \beta_2 X_{ma} + \beta_3 X_{ne} + \beta_4 X_{sa} + \beta_5 X_g + \beta_6 X_{pa} + \beta_7 TT + \beta_8 INC + \beta_9 VG + \beta_{10} IMP + \varepsilon$$

where P_{ch} is the annual real price in Chesapeake Bay, X are per capita annual landings subscripted by the producing region (ma=Mid-Atlantic, other than Chesapeake; ne= New England; sa = South Atlantic; g= Gulf of Mexico), TT is a year time trend variable, INC is real per capita disposable income, VG is zero for the period 1975-1990 and is equal to the per capita Gulf production for 1991-2006, IMP are imports of fresh/frozen or fresh/frozen/salted/brine oyster products, α and β's are parameters to be estimated and ε is the error term. The model was estimated using ordinary least squares.

The revised model has a significantly greater own price flexibility than was original estimated, so these new results require significant updating of the projections in the EIS. The model explanatory power actually increases to an r^2 of 0.89 (n=32) from an r^2 of 0.75 (n=57).

Table 1. Model results from inverse demand for Chesapeake Bay oyster production.

	Coefficients	*Standard Error*	*t Stat*
Intercept	-0.77543	2.24424	-0.34552
X_{ch}	-26.14640	6.50971	-4.01652
X_{ma}	91.12326	28.38465	3.21030
X_{ne}	17.98797	13.15814	1.36706
X_{sa}	-24.96263	52.19655	-0.47824
X_g	-7.12168	4.34319	-1.63973
X_{pa}	30.61914	11.96673	2.55869
INC	0.00042	0.00019	2.27427
VG	-17.52835	4.66819	-3.75485
IMP	-31.35519	39.23607	-0.79914
TT	-0.15412	0.08389	-1.83713

In response to reviewer #1, the model was also run for the 1980-2006 period. The own price parameter changed only slightly, by less than 3%, and was not significantly different. The advantage of using the longer time period is that it includes some observations at higher levels of production that might be anticipated with a restored resource in Chesapeake Bay. This was the main rationale for using the 1950-2006 data, production in the original analysis. By the 1980's, Chesapeake production was only 37% of the average production of the 1950's, whereas, including some data from the 1970's allows us to include observations where production was still around 65% of the 1950's level. Production from 2000-2006 was only 2% of the 1950's level. The 1975-2006 time period seems like a reasonable compromise to trade-off accounting for structural shifts and including observations near the level at which production projections are going to be made for the analysis. Using only the most recent data on which to estimate own price flexibility would be equivalent to not expecting the industry to adjust at all to the development of a 25-fold increase in production from Chesapeake Bay that might occur over a ten-year period.

The revised specification does not solve the issue that oysters from certain regions appear to be complements and not substitutes for Chesapeake produced oysters. Thus, oysters that are produced north of the Chesapeake appear to be complements, south of the Chesapeake are substitutes, and Pacific oysters are complements. While differences are expected in the markets for these oysters, they would all be expected to be substitutes, although relatively less substitutes than Gulf of Mexico oysters. The problem is that these are not homogeneous products. New England and Mid-Atlantic oysters are primarily sold in the halfshell market, commanding a higher price. Chesapeake oysters, like the Gulf of Mexico and South Atlantic serve more of the shucked market. Pacific oysters are also heavily used in the shucked market, and of course, are a different species. We also know that Chesapeake processors import shellstock from the Gulf of Mexico for local shucking and also purchase shucked Pacific oysters for repackaging, but no estimate of this product movement is readily available. Thus, the definition of regional markets becomes quite convoluted. The market is complex and the average ex-vessel prices and landed quantities in a region do not capture all that is occurring.

As reviewer two points out, the possibility of running scenarios where Pacific oysters increase significantly, thus, increasing predicted Chesapeake oyster prices is possible. However, the use of the demand model in the EIS is only for a partial analysis of the change in Chesapeake production. Production in all other areas is fixed in the simulation at the average for the last five years of production. Thus, we are implicitly assuming that there will be no change in imports, or U.S. production in other regions during the time-frame when Chesapeake oysters are increasing.

The average production from New England and the Mid-Atlantic is an order of magnitude smaller compared to the production from the Chesapeake, Gulf or Pacific (Table 2). The net impact on Chesapeake price is thus very small. Pacific oysters, on the other hand, play a more significant role.

Table 2. Average per capita oyster production by region (1975-2006)

	CH	MA	NE	SA	GU	PA
Per capita	0.035749	0.005408	0.007209	0.005293	0.082141	0.035438

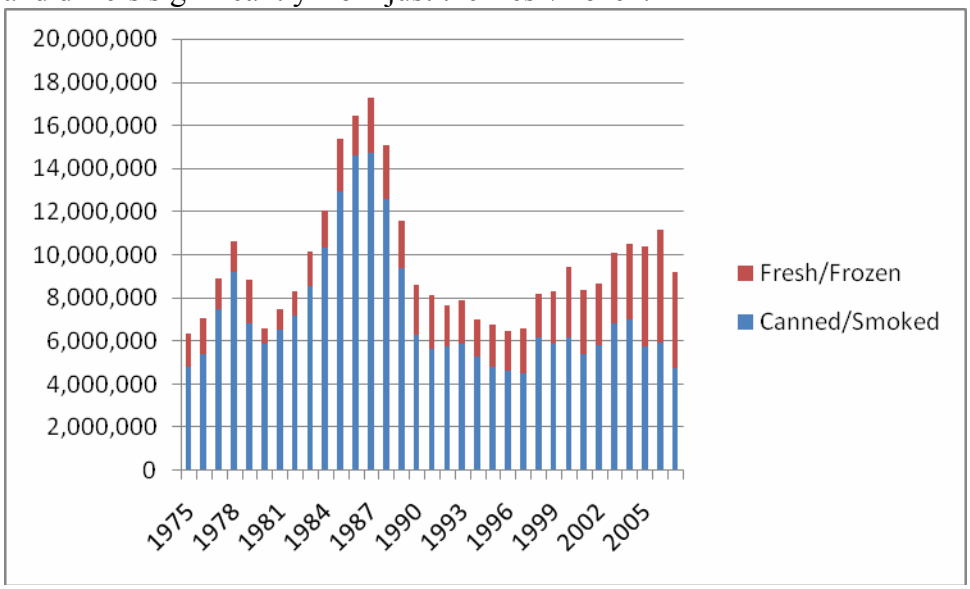
To explore this issue further, I ran the model without Mid-Atlantic and New England production. One could make the argument, that their production in inconsequential to the Chesapeake

market. In fact, most of this product is sold in the Northeast region and does not make it to the Chesapeake. The results are very insensitive to this change; with a 0.2% change in the estimation of Chesapeake own production parameter. Explanatory power declines from 0.89 to 0.82. I also ran the model without Pacific oyster production to see how it would affect the Chesapeake parameter. The absolute value of the Chesapeake parameter increases by 8% which is not a statistically significant change. Interestingly, dropping the Pacific production variable had virtually no affect on the R^2 value.

After exploring the model in detail, I feel the adopted model is a reasonable approach to estimating the own price effect of increased Chesapeake production to help guide the EIS. This author and, I believe, the reviewers agree that projecting relationships significantly outside the range of recent experience offers a significant challenge. Thus, it is important that what is presented in the EIS has the appropriate caveats that it is unlikely that any projection will be able to represent the true value. The approach adopted is most useful in comparing the relative performance of alternatives within the EIS rather than an absolute predictor of what will occur.

Doug Lipton
May 7, 2008

Figure 1. Oyster imports broken down into canned/smoked and fresh/frozen. The pattern of imports for the aggregate product is basically driven by changes in the canned/smoked product and differs significantly from just the fresh/frozen.



June 11, 2008 Third Peer Review Report

Lipton D. (2008). Economic Benefits of a Restored Oyster Fishery in Chesapeake Bay
Lipton, D. (3/16/2008). Final Draft Economic Analysis for Oyster Restoration Alternatives
and
Lipton D. (2008). Background Supporting Document B - For Economic Analysis of Alternatives: Projecting Oyster Harvests

From: James L. Anderson, Chair Peer Review Group
Department of Environmental & Nat. Res. Econ.
University of Rhode Island

Re: Evaluation of Lipton's Response to the Peer Review Group Comments

The members of the expert panel and I have carefully read Lipton's response to our comments, and we evaluated the changes that he made to the various documents. Lipton has adequately addressed the groups' concerns. As with all models, Lipton's work has some limitations; however, we have observed that his work is better than most being used in other EIS studies. We do not recommend any additional changes.